

The Applicability of the Fraud on the Market Presumption to Analysts' Forecasts

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The application of the fraud on the market presumption to security analysts' forecasts requires that those forecasts materially influence share prices in an efficient market. We provide evidence on the pervasiveness of reliably (i.e., statistically significant at conventional levels) unusual (larger than movements that occur on non-event days) price responses to analysts' forecasts. Relative to the average price movement on non-event days, we find that the mean price response to analysts' forecasts is reliably unusual, and the median response is not (in fact, the median indicates smaller price movements on forecast days than on the average of non-event days). In contrast, price responses to earnings announcements and to management forecasts exhibit much more pervasive reliably unusual price responses. Regardless of how we partition forecasts (firm-year, analyst-firm, analyst-year, or analyst-firm-year), we find that the majority of price responses are not reliably larger than those observed on average non-event days, and for the sample with fewest confounding events, the incidence of *insignificant* price responses is between 86% and 99%. Overall, we interpret these results as providing evidence against extending the fraud on the market presumption to analysts' forecasts.

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1. Introduction

This paper provides evidence on the applicability of the fraud on the market presumption of reliance to analysts' forecasts. Under this presumption, the starting point in litigation over allegedly bad information put into the marketplace by analysts is that (1) the bad information distorted share values and (2) investors who relied on the integrity of share prices in making investment decisions were thereby defrauded. The underlying assumption for this chain of reasoning is that share prices respond to and incorporate analysts' statements of opinion (their stock recommendations and earnings forecasts, for example). We give empirical content to this assumption by asking whether share price movements on analysts' forecast days are sufficiently large and pervasive across broad samples to support a conclusion that there is a high probability that any given forecast drawn from a comparable sample would be associated with a material share price response.

As discussed in more detail in section 2, our investigation is motivated by conflicting evidence and assertions about the applicability of the fraud on the market presumption to analysts' statements of opinion and by the importance of this presumption for class certification, which in turn has a significant influence on the outcome of litigation.¹ With regard to the applicability of the presumption, Judge Rakoff of the Southern District Court of New York concluded that because analysts express subjective and uncertain opinions—not facts—and because their reports are often issued at the same time as firm-initiated disclosures and conflicting opinions of other analysts, it should not be presumed that a given analyst's statements would materially affect share price; his view is shared by Coffee [2001]. On the other hand, Judge Lynch of the same district court found that ex ante evidence (such as statistical studies of how analysts influence share values) combined with logical arguments yields the opposite inference.

¹ If the fraud on the market presumption applies, the class of plaintiffs can include all those who bought (and did not sell) during the period when security prices were allegedly distorted by bad information (because the fraud on the market presumption means investors can rely on the integrity of price in making their decisions). If the presumption does not apply, then *each* plaintiff would have to show reliance on the alleged bad information. Clearly, the damages that could be claimed if the fraud on the market presumption applies far exceed the damages that could be claimed if individual showings of reliance on allegedly bad information are required.

In addition, an *amicus curiae* brief filed by the Securities and Exchange Commission (SEC) argues forcefully that the fraud on the market presumption applies to analyst reports. Finally, research examining the materiality of market reactions to analysts' reports (reviewed in section 2) finds a reliably non-zero average market response.

We provide empirical content to this debate by examining whether the reliably non-zero average share price impact of analysts' forecasts (documented in prior research) is driven by a small subset of observations or whether the results are pervasive. We assess pervasiveness in two ways. The first approach, based on the distribution of price responses to analysts' forecasts, evaluates pervasiveness as the proportion of the distribution that displays a reliably unusual response—a greater proportion of unusual responses indicates greater pervasiveness. The second approach compares the *relative* pervasiveness of unusual price responses to analysts' forecasts to similar measures for earnings announcements and management forecasts. Our motivation for the latter derives from two sources. First, the fraud on the market presumption has been applied without much question to management forecasts and earnings announcements so a finding that price responses to analysts' forecasts are about as pervasive as price responses to these two other types of information releases would support applying the presumption to analysts as well. Second, in questioning the applicability of the fraud on the market presumption to analysts' forecasts, Judge Rakoff characterized analysts' forecasts as (merely) statements of opinion made by third parties, in contrast to earnings announcements—statements of fact made by issuers—and management earnings forecasts—statements of opinion made by issuers.

We investigate the distributional properties of price response measures for a broad sample of over 2 million analysts' disseminations of forward-looking information (earnings forecasts, growth forecasts and stock recommendations, hereinafter, forecasts) made during 1990-2003, encompassing forecasts issued for firms listed on the NYSE, AMSE or NASDAQ. Our data consist of explicit forecast dates, which allows us both to examine single-day event windows and to isolate the effects of confounding events (such as earnings announcements or other analysts' forecasts) occurring on the same day as the forecast. Based on Judge Rakoff's comments about confounding events and conflicting analyst opinions,

we report results for samples that exclude event days with other arguably pertinent announcements and days when more than one analyst issued a forecast.

To abstract from the type of news (good or bad) conveyed by analysts' forecast, we examine absolute price reactions. To identify *statistically unusual* (i.e., material) price movements, we measure a forecast's stock price impact as the difference between the absolute value of the firm's standardized market model residual on the forecast day and the average value of that firm's absolute standardized market model residuals on randomly-selected non-event days.² We define non-event days as days on which no analyst forecast was issued and no earnings announcement, ex dividend day, or management forecast announcement occurred.³ A positive (negative) difference—that is, a positive (negative) price response measure—indicates that the forecast day is associated with a larger (smaller) price movement than occurs on non-event days.

To provide evidence on whether analysts' forecasts are associated with large and pervasive price responses, we provide two kinds of analyses. First, we analyze the distribution of price response measures for analysts' forecasts to determine what portion of the distribution is characterized by reliably unusual price responses. We find that the mean price response measure is significantly positive, indicating that *on average* share price movements on analysts' forecast days exceed the average share price movement on other days, while the median price response on forecast days is significantly *negative*, meaning that more than half (specifically, about 55%) of the time less new information reached the market on forecast days than on the average of non-event days. When we exclude forecasts which occurred on the same day as a confounding event (i.e., earnings announcements, ex dividend days, and management forecasts) and/or on the same day as other forecasts made about this firm, the average price

² As discussed in more detail in section 2.4 and in our analysis of results, we also use the median standardized market model residual on non-event days as a benchmark, and we use a non-parametric test based on the distribution of non-event day price responses.

³ As discussed in section 2.2, a “non-event” day is not necessarily a day with no disclosures. We exclude analysts' forecasts, earnings announcements, dividends, and management forecasts but we do not control for other information disseminations. Our price response measure should, therefore, be interpreted as providing evidence on whether analysts' forecasts move share values more than news which comes out on days when there are no public disclosures of analysts' forecasts, earnings announcements, dividends, and management forecasts. We revisit this issue in section 4 where we examine the incidence of other types of disclosures on random samples of non-event days and forecast days.

response becomes less positive, the median response becomes more negative, and the frequency of negative price responses increases. In tests which use the median standardized market model residual on non-event days as the benchmark, we find that both mean and median results are positive (about 47% of the price response measures are negative for the sample that eliminates confounding events).

Second, we compare these results to the distributions of share price responses to earnings announcements and management forecasts issued by the same sample of firms. We find that mean price responses are positive for both earnings announcements and management forecast days, and much larger than the responses to analysts' forecasts. When the benchmark for non-event days is the mean standardized residual, median price responses are also positive, with about 58% of earnings announcements and about 70% of management forecasts having positive responses. Excluding confounding events has little impact on the results for earnings announcements and increases (to about 72%) the portion of management forecasts associated with positive responses. Using the median non-event day standardized residual as the benchmark increases the evidence of positive price responses; about 66% (77%) of price responses to earnings announcements (management forecasts) are positive. Overall, these results indicate that share price responses to earnings announcements—statements of fact made by issuers—and to management forecasts—statements of opinion made by issuers—are both more material and more pervasive than are share price responses to analysts' forecasts.

We also analyze subsets of analysts' forecasts where we aggregate forecasts at the firm-year level, the analyst-firm level, the analyst-year level, and the analyst-firm-year level. This analysis sheds light on whether our results based on over two million forecast days, each viewed as an independent observation, mask certain patterns in the data. For each partition, we limit the analysis to cases with at least 10 unique forecast days so that we can reliably assess the statistical significance of price response measures. For example, in the analyst-firm partitioning, the unit of observation is an analyst-firm pairing, and the price response measure and its associated dispersion are derived from the 10 or more forecast days during the period 1990-2003 on which that analyst issued forecasts for that firm.

For the partitioned samples, we find that the pervasiveness of the share price response to analysts' forecasts differs depending on the statistic analyzed (mean versus median), the partition chosen, the benchmark measure of non-event day standardized residuals (mean versus median) and the inclusion or exclusion of confounding events and multiple analyst forecasts. The strongest evidence of a pervasive price response to analysts' forecasts is based on tests which use as the benchmark the *median* non-event day standardized residual; for these tests as much as 65% of the sample partitioned by analyst-year shows a positive price response measure (with 7.6% (11.9%) significant of the measures at the 5% (10%) level). In contrast, tests which use the *mean* non-event day standardized residual as the benchmark show at best 32% positive, supporting a general inference that the majority of price responses to analysts' forecasts are not reliably larger than average price responses observed on non-event days. For example, at a 5% significance level, the percentage of reliably positive price responses is between 8.9% (partition on analyst-firm-year) and 44.1% (partition on analyst-year) for mean values, and between 2.2% (partition on analyst-firm-year) and 6.2% (partition on analyst-year) using median values. When we exclude confounding events and days with multiple analyst forecasts, the proportion of mean (median) price responses that are reliably positive, for any partition, is less than 13% (less than 2%).

In summary, consistent with prior research, we find evidence of a significant average price response to analysts' forecasts. The average response is not, however, systematically descriptive of the rest of the sample—that is, the positive response is not pervasive across the sample—and even the average response is contaminated by confounding events. The majority of price responses to analysts' forecasts are not associated with price movements that are reliably different from the average standardized price movement on non-event days, suggesting that these forecasts do *not* convey materially new information to the market—regardless of whether we consider the population of forecasts or partition by firm-year, by analyst-firm, by analyst-year, and by analyst-firm-year. If the comparison is between the price response to analysts' forecasts and the *median* price response on non-event days, there is more evidence of a pervasive response. However, in no case is the price response to analysts' forecasts as strong as, or as pervasive as, the price responses to earnings announcements and management forecasts.

These results are robust to a number of sensitivity tests. Overall, we interpret the results as supporting the view that the fraud on the market presumption, which has been applied without (much) controversy to earnings announcements and management earnings forecasts, would not extend to analysts' forecasts.

The rest of the paper is structured as follows. In section 2 we describe conflicting views about the applicability of the fraud on the market presumption to analysts' forecasts. Section 3 reports the results of our main tests, and section 4 considers additional sensitivity tests. Section 5 summarizes the results and concludes.

2. Analysts' Forecasts, Share Price Responses and the Fraud on the Market Presumption

This section begins by discussing why the fraud on the market presumption is important for securities litigation (section 2.1). We then present qualitative arguments which support and refute the applicability of the fraud on the market presumption to analysts' forecasts and link these arguments to our tests (section 2.2). We summarize previous research on price responses to analysts' forecasts and place our study in the context of this literature (section 2.3), and conclude (section 2.4) by describing several methodological issues that arise from, or are related to, the qualitative arguments presented in section 2.2, and explaining how our research design addresses these issues.

2.1. The importance of the fraud on the market presumption.

By *presumption*, we mean a legal device that produces certain inferences from the available facts and evidence, as long as other facts (or evidence) are not produced to contradict those inferences.⁴ In a securities case involving specific allegations that defendants provided defective information, application of the fraud on the market presumption allows a large class of plaintiffs to meet the reliance requirement by arguing that investors reasonably relied on price as an information signal in making their investment decisions.⁵ That is, if the presumption is applicable, individual plaintiffs need not show that they directly

⁴ For example, in criminal cases, defendants are *presumed innocent* until proven guilty; in corporation law, directors of corporations are *presumed to have made reasonable decisions* under the business judgment rule.

⁵ In alleging securities fraud, plaintiffs are typically required to plead a version of the following five part argument: (1) defendants made a false statement (or omitted information) with regard to a material fact; (2) in connection with

read and used (relied on) allegedly defective information provided by defendants in making investment decisions; rather that reliance is *presumed* for an entire class of investors who transacted in the security during the period when prices were allegedly distorted by bad information, if the securities that are the subject of the litigation were traded in an efficient market in which prices reflect all pertinent information.

The question of the applicability of the fraud on the market presumption is not connected to the merits of the case, *per se*.⁶ The question of whether the presumption should apply is addressed at the class certification stage of securities litigation—months before any trial of the facts begins. The economic impact of applying the fraud on the market presumption is significant because of the effects on the size of the plaintiff pool, and, therefore, the total amount of damages that can be claimed. That is, if the presumption is applied, the class can contain all investors who bought and held securities during the period when price was allegedly distorted and the calculation of damages would consider the losses sustained by the entire class. If the presumption is not applied, each plaintiff must individually demonstrate reliance on the alleged bad information supplied by defendants and the calculation of damages would be based on each plaintiff's individual losses.

The importance of applying the fraud on the market presumption as a determinant of outcomes is well recognized in the legal literature.⁷ If the presumption is applied, defendants would have to refute, in a trial of the merits of the case, one or more elements of the presumption (e.g., the securities did not trade in an efficient market; the allegedly bad information was not material). Class certification achieved by applying the fraud on the market presumption, therefore, significantly increases pressure on defendants to

the purchase or sale of a security; (3) with scienter [intent]; (4) plaintiffs reasonably relied on the defective information provided by defendants; and (5) the defendants' false statement (or omission) caused the alleged loss.

⁶ The merits of the case include, for example, loss causation (did the allegedly defective information cause damage to the plaintiffs). Our analysis is not intended to speak to loss causation. However, if an inflated price at the time of purchase is a *necessary* condition for loss to occur (as might be inferred from the Supreme Court's ruling in *Dura Pharmaceuticals v. Broudo*, 125 S. Ct. 1627 (2005)), a finding that analysts' forecasts do not affect share prices has implications for loss causation in situations where the allegedly defective information is an analyst's statement. For additional discussions of the Supreme Court's decision, see, for example, Fox (2005).

⁷ For example, historically "most securities class actions are resolved through settlement if the plaintiffs successfully jump the class certification hurdle" (Flumenbaum and Karp, 2005). See also *Coopers & Lybrand v. Livesay*, 437 US 463, 476 (1978) ["Certification of a large class may so increase the defendant's potential damages liability and litigation costs that he may find it economically prudent to settle and abandon a meritorious defense"], and *Blair v. Equifax Check Services* 181 F.3d 832, 834 (7th Circuit 1999) ["a grant of class status can propel the stakes of a case into the stratosphere" thus providing a "device to wring settlements from defendants"].

settle and pay a portion of the claimed damages, as opposed to taking a chance on a ruinous outcome in a trial of the merits of the allegations.

2.2. Qualitative arguments pertaining to the applicability of the fraud on the market presumption to analysts' forecasts.

There are several reasons to believe that the fraud on the market presumption should apply to security analysts' statements of opinions.⁸ First, in adopting the fraud on the market presumption of reliance in *Basic v. Levinson* (485 U.S. 224, 1988), the Supreme Court did not distinguish among sources of information; that is, the Court simply referred to material misrepresentations or omissions. Therefore, there is no *ex ante* reason to believe that information provided by third parties, such as analysts' forecasts of earnings, sales or growth rates, would be treated differently from information provided by issuers, such as management forecasts of earnings, sales or growth rates—both represent informed judgments and opinions about the future. Second, analysts are paid to serve as sophisticated processors and synthesizers of information that is pertinent to investment decisions, so it would be expected that investors would view analysts' informed opinions as valuable inputs to their decisions and that markets would impound those opinions in security prices. Third, there is anecdotal evidence of large price movements in response to analysts' statements⁹ and previous research (described in section 2.3) supports the view that analysts' opinions move stock prices on average.

A contradictory view—that the fraud on the market presumption of reliance *should not* apply to analysts' statements of opinions—was articulated by Judge Rakoff in a recent ruling of a class-action certification of a case involving allegedly false and misleading statements made by a security analyst:¹⁰

⁸ A number of these reasons are discussed in the *Brief of the Securities and Exchange Commission, Amicus Curiae*, submitted to the Second Circuit Court of Appeals in connection with the WorldCom securities litigation. The brief is available at www.sec.gov/litigation/briefs/wchevesi_amicus.htm

⁹ For example, Henry Blodgett's December 16, 1998 prediction that Amazon.com's stock price would reach \$400 in 12 months was credited with an immediate 19% increase in Amazon's share price (*The Washington Post*, December 17, 1998, p. B01). On the negative side, CNNfn reported on August 29, 2000 that Yahoo! lost nearly 9% after an unnamed Lehman Brothers analyst expressed concerns about the firm's third quarter 2000 performance.

¹⁰ *DeMarco v. Lehman Brothers, Inc.* Plaintiffs claimed that at the same time that Lehman analyst Stanek was issuing a strong buy recommendation on RealNetworks in his public reports, he was providing selected clients with advice to sell this stock. Plaintiffs sought class certification, arguing that Stanek's false public statements constituted a fraud on the market and, in the spirit of *Basic v. Levinson*, 485 U.S. 224 (1988), can be presumed to

... [T]here is a qualitative difference between a statement of fact emanating from an issuer and a statement of opinion emanating from a research analyst. A well-developed efficient market can reasonably be presumed to translate the former into an effect on price, whereas no such presumption attaches to the latter. This, in turn, is because statements of fact emanating from an issuer are relatively fixed, certain, and uncontradicted. Thus, if an issuer says its profits increased 10%, an efficient market, relying on that statement, fixes a price accordingly. If later it is revealed that the previous statement was untrue and the profits only increased 5%, the market reaction is once again reasonably predictable and ascertainable. By comparison, a statement of opinion emanating from a research analyst is far more subjective and far less certain, and often appears in tandem with conflicting opinions from other analysts as well as new statements from the issuer. As a result, no automatic impact on the price of the security can be presumed and instead must be proven and measured before the statement can be said to have “defrauded the market” in any *material* [emphasis added] way that is not simply speculative.

This ruling appears to rest on a characterization of the distinctive *nature* (opinion, not fact), *source* (third party, not issuer) and *information environment* (often released concurrently with other news) of analyst forecasts. Taken together, this characterization calls into question whether it is justifiable to assume, *ex ante*, that price responses to analysts’ forecasts are *material*. We consider each aspect of this characterization in turn, beginning with materiality.

To analyze the materiality of price responses to analysts’ forecasts, we compare a measure of share price movements on analyst forecast days to a measure of price movements on “normal” trading days which we define as days with no analysts’ forecasts, no earnings and dividend announcements and no management forecasts; we use both the mean and the median price movement on these “normal” trading days as benchmarks. Implicit in this comparison is a definition of materiality: a forecast would be considered to have caused a *material* price movement if the price movement on the forecast day exceeds the price movement observed on trading days when there are no significant issuer-initiated or analyst-initiated announcements (we refer to the latter as “non-event days”).

A finding of no material price movement does not imply that analysts’ reports provide no useful information to investors. Analysts’ activities can enhance stock market efficiency even if a typical analyst forecast has no material influence on share values. For example, Douglas [1933] argues that

affect the share price of the stock in question, and consequently, all investment decisions predicated on that share price. On July 6, 2004, the United States District Court of the Southern District of New York ruled in favor of defendants (WL 1506242 S.D.N.Y., 2004), refusing to grant class certification.

informed market participants (such as analysts) can and will acquire and process information that unsophisticated or time-constrained individual investors may not, so that the information will ultimately be reflected in share prices. However, each bit of information need not have a material influence on share prices, because the materiality of the price response to any event is determined both by the amount of news conveyed (a function of the market's expectations about the event) and by the underlying volatility of the stock. Analyst reports that convey small amounts of new information will not move stock prices by meaningful amounts even though the new information is impounded in prices. Furthermore, reports conveying large amounts of new information will not result in material price responses if the volatility of the stock's returns is so large that it is not possible to detect a statistically significant difference between price movements on report dates from price movements at non-event times.

Turning to the nature and source of analysts' forecasts, we infer that Judge Rakoff's characterization rests on the view that statements of fact made by issuers are presumptively more likely to affect share values than are statements of opinion made by third parties such as analysts. We analyze this view by comparing the distributions of price response measures for analysts' forecasts with the distributions of price response measures for statements of fact made by issuers (earnings announcements) and for statements of opinion made by issuers (management earnings forecasts). We choose earnings announcements and management earnings forecasts both because the fraud on the market presumption has been applied to these disclosures and because they pertain to the same type of information (that is, earnings information) that is the subject of many analyst disseminations. We measure the price responses to these issuer-initiated statements in the same way they are measured for analysts' forecasts – as the difference between the price response on the event day and the price response on non-event days.

Finally, we analyze Judge Rakoff's characterization of analysts' statements of opinion as often appearing concurrently with other news, including contradictory views, from other analysts and the issuer. The implication is that what appears to be a price response to a specific analyst's forecast may in fact be a response to other information released concurrently. We provide empirical content to this view by

analyzing the distributions of share price responses to analysts' forecasts including and excluding days with other forecasts and certain concurrent news releases.

We also consider another reason—not discussed by Judge Rakoff—why the fraud on the market presumption might not be applicable to analysts' reports. Specifically, we examine the possibility that the *average* price response to analysts' forecasts—calculated over a large sample of firms and many years—might not be indicative of a *typical* price response. This would be the case if the distribution of responses to analysts' forecasts is skewed so that the mean is strongly affected by a relatively small number of extremely large responses. We analyze this possibility by studying (in addition to the mean) the median, skewness and kurtosis of the distribution of price responses to analysts' forecasts.

To summarize, qualitative reasoning as expressed in various legal arguments and rulings does not unambiguously support or refute the applicability of the fraud on the market presumption to analysts' statements of opinion. Judge Rakoff's ruling implies that the fraud on the market presumption would apply if analysts' opinions can be proven to be part of the pertinent and material information that is reflected in share prices, so that those opinions affect all investment decisions that are predicated on share prices. We seek to provide evidence on this issue by investigating whether analysts' forecasts are systematically and pervasively associated with statistically reliable price responses (that is, whether analysts' forecasts are, in general, material to investors). Evidence of systematic and pervasive responses supports the assumption that any given analyst's forecasts would materially affect share prices.

2.3. Previous research on share price responses to analysts' forecasts.

Early research documents a statistically reliable *average* price response to security analysts' revisions of earnings forecasts and revisions of stock recommendations. Specifically, researchers find significant positive (negative) movements in stock prices in response to upward (downward) revisions in analysts' earnings forecasts (Givoly and Lakonishok [1979]) and stock recommendations (Elton, Gruber and Grossman [1986]; Womack [1996]). Research has also examined price reactions to components of analysts' reports (see, for example, Francis and Soffer [1997] and Asquith, Mikhail and Au [2004]), finding that the market responds, on average, to all components of the analyst's report (i.e., earnings

forecast revisions, revisions in stock recommendations and price targets, and qualitative justifications for those revisions). Much of the subsequent research on analysts' forecasts takes the existence of a price response as a starting point, and examines how these price responses correlate with characteristics of the report, the analyst, and the firm. For example, Lys and Sohn [1990] document a positive association between the magnitude of the price response and the magnitude of the earnings forecast revision. Research on the relation between price responses to analyst reports and measures of analyst ability or reputation (e.g., Stickel [1992]; Mikhail, Walther and Willis [1997], Park and Stice [2000]; Chen, Francis and Jiang [2005]; Bonner, Hugon and Walther [2005]) generally finds that price responses are bigger to reports issued by more able or better known analysts (defined as analysts named to the *Institutional Investor* All-American research team, with more experience, with better and longer track records, or with more media coverage). Finally, Frankel, Kothari and Weber [2004] investigate the firm-specific determinants of the informativeness of analysts' forecasts. They find that analyst reports are associated with larger absolute price reactions when reports are issued for stocks with higher trading volume, returns volatility and institutional ownership, and less complex business models.

In summary, prior research provides broad sample evidence on whether there is a reliably non-zero *average* price response to analysts' reports and on whether and how share price responses to analysts' reports correlate with report-specific, analyst-specific, or firm-specific variables. Prior studies have not, however, probed the distributional properties of the share price response, so it is unknown whether the significant average price response is descriptive of most forecasts or is driven by a potentially small subset of forecasts. Consequently, the results of this body of work are not directly on point (and therefore are not dispositive) for addressing the question of whether the fraud on the market presumption should apply to analysts' forecasts.

Our study addresses this gap by examining the pervasiveness of reliably unusually price responses to analysts' forecasts. Our examination proceeds along two dimensions. First, we provide large-sample evidence concerning investor responses to analysts' forecasts. In this sense, our study re-examines some of the earliest findings in the analyst forecast literature, documented by Givoly and

Lakonishok [1979] (GL), concerning stock price movements surrounding analysts' forecasts. GL use a sample of 1,420 annual earnings forecasts over 1967-1974, issued by the most active forecasters (based on frequency of forecast activity as reported in the Standard and Poor's Earnings Forecaster) for 49 NYSE-listed, December-year end firms in three industries (Chemicals, Petroleum Refining, and Transportation Equipment). They report average abnormal returns in the month of the forecast announcement of about 1%, and about 0.8% when months containing earnings announcements are excluded; both are significant at the 5% level.¹¹ Our re-examination uses a more comprehensive sample of over 2 million analysts' forecasts made over 1990-2003: we include all analysts' forecasts issued for all firms listed on the NYSE, AMSE or NASDAQ, and we do not restrict the sample to firms with December fiscal year ends. Our forecast data, collected from both Zacks and First Call, has explicit forecast dates (as compared to the weekly report dates of the S&P data used by GL) which allow us to narrow the event window for the forecast announcement to the market response on day 0 (the forecast date). An advantage of narrowing the event window (which we exploit in our tests) is the ability to isolate the effects of other events (such as earnings announcements or other analyst forecasts) that also occurred on the forecast date.

Second, we partition our sample in ways that allow us to probe the statistical significance of various aggregations of analyst forecasts. These tests permit us to speak to, for example, the question of what fraction of forecasts made about a given firm in a given year (firm-year aggregation) or made by a given analyst in a given year (analyst-year aggregation) elicits *reliably* unusual share price responses. In total, we consider four levels of aggregation (firm-year, analyst-year, analyst-firm, and analyst-firm-year). We illustrate the partitioning by considering analysts following Amazon.com. The *firm-year* tests speak to whether the analysts following Amazon.com in 1995 significantly moved Amazon's share price. The *analyst-firm* tests speak to whether a specific analyst, say Jack Grubman, issued forecasts for Amazon.com over 1990-2003 that significantly moved share price. The *analyst-year* tests speak to

¹¹ GL separate their sample based on whether the forecast was revised, the amount of the revision, and its direction. For positive forecast revisions of at least 5% of the prior earnings forecast, they find an average abnormal return of 0.9% (0.7% excluding months with earnings announcements). For negative forecast revisions of at least 5% of the prior earnings forecast, they find an average abnormal returns of -1.2% (-0.9% excluding months with earnings announcements).

whether Jack Grubman’s forecasts in 1995, for all firms he followed, induce significant share price responses. Finally, the *analyst-firm-year* tests speak to whether Jack Grubman’s forecasts for Amazon.com in 1995 significantly influenced share price. In summary, then, the aggregation tests provide evidence about whether the conclusion from the early forecasting literature – that analysts’ forecasts significantly influence share prices on average – is applicable to most firm-years, most analyst-firm pairings, most individual analyst-years, and most individual analyst-firm-years.

2.4. Methodological issues in providing evidence on share price responses to analysts’ forecasts

Showing that analysts’ statements have a material influence on share price requires two types of empirical evidence.¹² The first is that stock prices moved by unusual amounts (i.e., more than they typically move in the absence of statements by analysts). The second is that the statement being examined, and not something else that happened concurrently, is the source of the stock price movement. Obtaining both types of evidence is complicated by several methodological issues, all of which are implied by the arguments for and against the existence of such an influence.

Evidence that stock prices moved by unusual amounts. Obtaining evidence of unusual price movements requires a price response measure that compares the price response to an analyst forecast to what would be expected in the normal course of events, that is, a benchmark. Our price response measure focuses on the difference between the absolute standardized market model residual on forecast days and the absolute standardized market model residual on non-event days in the same firm-year.¹³ As benchmarks for what would be expected on a typical “non-event” day (where there are no known significant issuer-initiated or analyst-initiated announcements), we use the *average* and the *median* absolute risk-adjusted standardized (by volatility) price movement on non-event days.

Both the mean and median benchmarks explicitly control for underlying returns volatility by standardizing the absolute value of the market model residual by the standard deviation of the firm’s

¹² The standard for materiality is taken from *TSC Industries v. Northway Inc.* (426 U.S. 438, 449 (1976)). Something is material “if there is a substantial likelihood that a reasonable shareholder would consider it important” in making an investment decision.

¹³ In sensitivity tests (discussed in section 4), we verify that our results are not sensitive to the specific price impact measure that we use.

market model residuals in year Y. This control is especially important, because some claims of shareholder losses due to security analysts' alleged misrepresentations have focused on reports issued about technology firms, which generally are characterized by high returns volatility. These include Salomon Smith Barney analyst Jack Grubman's reports about WorldCom (*In re WorldCom, Inc. Sec. Litig.*, 219 *F.R.D.* 267, *S.D.N.Y.*2003); Lehman Brothers analyst Michael Stanek's reports about RealNetworks (*DeMarco v. Lehman Bros.*, 309 *F.Supp.2d* 631, *S.D.N.Y.*2004); and numerous reports issued by analysts employed by Lehman Brothers, Goldman Sachs and Morgan Stanley about RSL (*Fogarazzo v. Lehman Bros., Inc., Goldman Sachs, and Morgan Stanley, No. 3 Civ. 5194 (SAS), 2004 WL 1151542 S.D.N.Y. 2004*). The importance of controlling for volatility in a benchmark is underscored in the Demarco v. Lehman ruling which stated that the results of studies examining analysts' impact on the pre-Internet stock market have "little relevance" for assessing the impact of analysts' reports issued about Internet-related stocks during the Internet bubble and later collapse.

Both the mean and median benchmarks also explicitly control for price movements on non-event days. This control is necessary because we abstract from the sign of price responses and examine the absolute values of residuals. Both squared residuals and absolute residuals have a non-zero mean and do not follow a normal distribution (prior research shows they are leptokurtic, e.g., Marais [1984]). Therefore, non-zero price responses measured as absolute residuals are expected in the absence of material news—that is, that is, non-zero price responses are *typical* on non-event days. We capture the unusualness (that is, the materiality) of price responses to analysts' forecasts by comparing the absolute price responses to forecasts with a benchmark based on the empirical distribution of absolute price responses. Our approach follows research which uses similar price response metrics to study the materiality of price reactions to earnings announcements.

The difference between the mean and median benchmarks, in terms of capturing the price response that would occur on "normal" trading days that do not contain earnings announcements, ex-dividend days, management forecasts or analysts forecasts, derives from the shape of the distribution of standardized residuals on those "normal" trading days. There will be no meaningful difference if the

distributions are symmetric, and the mean will exceed the median if the distributions are positively skewed (i.e., a right tail of relatively large price responses). The mean takes account of those relatively large price responses; the median does not. Therefore, the choice between the two measures as benchmarks depends on which measure is viewed as better capturing what would be expected on a typical non-event day. We use the mean as the benchmark in our primary analyses but also report results using the median (as well as a non-parametric test based on the entire distribution) for comparison purposes.

Evidence that specific statements caused the unusual price movements. A demonstration that analysts' forecasts have material share price effects requires abstracting from the influence of other disclosures made at the same time as the analysts' forecasts being examined. We consider two types of disclosures. The first is earnings announcements and other firm-initiated announcements that pertain to earnings and dividends. Unlike many settings where the incidence of concurrent disclosures is dismissed because of their expected infrequent and random occurrence in relation to the event being examined (e.g., concerns that market reactions to earnings announcements are contaminated by the effects of FDA drug approval announcements), concurrent disclosures are unlikely to be either infrequent or randomly distributed in the case of analysts' forecasts. In fact, we would expect other news (such as earnings and dividend announcements, FDA approvals, tender offers, and new product announcements) to *cause* analysts to revisit the assumptions in their reports, leading to heightened forecasting activity in response to these events. Evidence supporting this expectation is reported by Stickel [1989]. If forecasts are more likely to occur on other disclosure dates, we may incorrectly attribute a price response on day t to an analyst report issued that day when, in fact, the response is to the other disclosure.¹⁴

The second type of concurrent disclosure we consider is other analyst reports. There is a tendency for reports issued by analysts about the same firm to cluster in time, either because analysts respond to the same firm-initiated news releases (Stickel [1989]) or because they herd (Welch [2000]),

¹⁴ A possible way to separate the market reaction to same-day disclosures would be to identify the timing of each disclosure and to use intra-day trading data to isolate the market response to each disclosure. We do not adopt this approach because forecast databases do not contain time stamps for analyst reports, hence it is not possible to know when they were disseminated on day t .

Cooper, Day and Lewis [2001], Chen and Jiang [2005]). This clustering increases the difficulty of establishing a link between a given analyst's reports and observed price responses because of both an inference problem and a dependence problem. The inference problem is that it is difficult to parse out how much of a given day's return is attributable to a specific report about firm j when multiple analysts issue reports about firm j on the same day.¹⁵ The dependence problem occurs to the extent the researcher treats each analyst report made on a multiple forecast day as a separate event or observation.

Our research design explicitly addresses the methodological issues associated with obtaining both types of evidence needed to establish a material response to analysts' forecasts. First, comparisons of standardized absolute market model residuals on forecast days versus non-event days directly measure the statistical unusualness of price movements on days of analysts' forecasts. Second, we report results for samples that include and, separately exclude, analyst reports issued at the same time as earnings announcements, ex-dividend days and management earnings forecasts. These procedures are incomplete to the extent that other types of disclosures are both prevalent in the data and likely to cause forecast revisions. However, this incompleteness should bias towards finding reliably unusual market movements in response to analysts' forecasts because of the causal relation between other disclosures and analysts' forecasts. In particular, if other disclosures prompt forecast revisions, there will be a greater likelihood of other disclosures on forecast dates relative to non-event dates, leading to an upward bias in our measures of share price response for forecast days and a downward bias for non-event days. Because our tests focus on the difference between the forecast day metric and the non-event day metric, we are more likely to observe positive differences (i.e., forecast day metric larger than non-event day metric) which are indicative of unusual news content on forecast days. That is, we believe that contamination from

¹⁵ In a practical litigation setting, the inference problem is more severe than just the inference about which analyst report, among a set of several analysts' reports, influenced share price. In most fraud on the market claims, plaintiffs point to specific allegedly defective quantitative assessments, such as price targets or earnings forecasts, made by an analyst in his report. Most analyst reports contain, however, a significant amount of other information which often qualifies the valuation implications of quantitative assessments. One might argue, therefore, that the quantitative assessments should not be interpreted out of context of the mix of information contained in the analyst's report.

concurrent disclosures induces a bias towards a finding that analysts' forecasts significantly influence share prices. (Sensitivity tests, described in section 4, confirm this conjecture.)

To address the inference and dependence issues associated with analyst report clustering, we report the results of tests conducted on all forecast days (which include days with a single-forecast and days with multiple-analysts) and, separately, on (only) single-analyst days. Results including multiple analyst days are biased towards finding significant market reactions, whereas results excluding multiple-analyst dates are biased against this finding. The reason for this bias is two-fold. First, if multiple analysts issue reports on the same day for a given firm, it is more likely that there is, in fact, material news to report on this day. Second, multiple disseminations made by different analysts (with different retail and institutional client bases) are likely to reach a larger investment audience than is a single dissemination, so the set of investors who might respond to the report is larger on multiple forecast dates. Our tests do not suffer from dependence arising from multiple forecasts issued on the same date because we treat the return on a firm-forecast day as the unit of observation, and in no test do we allow the same forecast day return to appear multiple times.

Given prior research, we expect to find a significant average impact for large samples of forecasts. Our aim is to provide evidence about the pervasiveness of this impact both in the sample of all individual forecasts and in samples of forecasts aggregated by combinations of firm name, analyst name and year. We expect that evidence of analyst influence will be weaker when we control for the effects of contamination, inference and dependence.

3. Empirical Work

3.1. Sample and variable definition

Our sample consists of all analyst disseminations (of earnings forecasts, growth forecasts, or stock recommendations) with forecast dates available on Zacks Investment Research database or Thomson First Call database during 1990-2003. We obtain stock return and ex-dividend date information from CRSP; earnings announcement dates from Compustat; and management forecast date

information from First Call. We combine analyst forecast data from two sources (Zacks and First Call) to increase the likelihood that we identify a very broad sample of forecasts issued during the sample period. Zacks contains data on both forecast dates and analyst identities; First Call has data on forecast dates, but not analyst identities. Instead, First Call identifies each forecast with the brokerage firm that issues the forecast. The absence of analyst identifier information for First Call forecasts introduces two complications to our tests. First, in tests which aggregate forecasts at the analyst level (e.g., by analyst-firm, analyst-year, or analyst-firm-year), we do not have data on the analyst name for First Call forecasts. We address this problem by using brokerage affiliation instead of analyst identity for First Call data. Brokerage affiliation assumes there is a unique analyst (from a given brokerage) following a given firm. We assess the sensitivity of our results to this issue by repeating our tests excluding all First Call data; results (not reported) yield similar inferences. Second, a subset of our tests focuses on the market influence of *Institutional Investor* All-American analysts; because these tests (described in section 3.4) require knowledge of the name of the analyst, we exclude First Call data because we are not able to identify whether a First Call forecast is issued by an All-American analyst or not.

Following convention, we exclude forecasts for penny stocks, defined as firms with an average market capitalization of less than \$10 million or an average stock price of less than \$5 per share (both in 2001 CPI-adjusted dollar terms). In unreported tests, we verify that our results are qualitatively similar if we retain these firms. The sample covers 7,480 firms and 47,930 firm-years, and represents earnings forecasts, long term growth forecasts and stock recommendations. The total number of unique forecast days is 2,009,078, implying that the average number of firm-year-days with forecast activity is 42 (2,009,078 ÷ 47,930 firm-years).¹⁶ For the 2,009,078 forecast days, 1,141,165 have only Zacks forecasts, 459,622 have only First Call forecasts, and 408,291 have both Zacks and First Call forecasts.

¹⁶ Note that because the unit of observation is a forecast day, we treat multiple disseminations (e.g., an earnings forecast revision, a change in growth forecast and a stock recommendation revision) on the same day as a single observation.

Our measure of the price impact of analysts' forecasts is the adjusted absolute standardized market model residual, $Adj\left|\varepsilon_{j,t} / \sigma_{\varepsilon_{j,t}}\right|$, on forecast day t. To calculate $Adj\left|\varepsilon_{j,t} / \sigma_{\varepsilon_{j,t}}\right|$ we first estimate a market model for each firm-year Y, Y=1990-2003:

$$R_{j,t} = \alpha_j + \beta_j(R_{m,t}) + \varepsilon_{j,t} \quad (1)$$

where $R_{j,t}$ is firm j's raw return on day t; $R_{m,t}$ is the value-weighted market return on day t. We then calculate the absolute value of the standardized residual, $\left|\varepsilon_{j,t} / \sigma_{\varepsilon_{j,t}}\right|$, for each trading day t in that firm-year, where $\sigma_{\varepsilon_{j,t}}$ is the standard deviation of $\varepsilon_{j,t}$ in year Y. $\left|\varepsilon_{j,t} / \sigma_{\varepsilon_{j,t}}\right|$ is similar to May's [1971] U-statistic which has been used to detect increases in volatility (a measure of whether an event conveys news) around news events such as quarterly earnings announcements. Rohrbach and Chandra [1989] show that May's U-statistic is more powerful than other statistics (such as Beaver's [1968] U-statistic) when the market model residuals are leptokurtic, as they are in our sample. Rohrbach and Chandra further show that the statistical significance of the price response measure should be assessed by reference to its empirical distribution (because the mean of the distribution is non-zero and firm-specific).

Following this logic, we adjust $\left|\varepsilon_{j,t} / \sigma_{\varepsilon_{j,t}}\right|$ by subtracting the *mean* value of $\left|\varepsilon_{j,t} / \sigma_{\varepsilon_{j,t}}\right|$ calculated for a random sample of non-event days in year Y, $mean\left|\varepsilon_{j,t}^{Non-event} / \sigma_{\varepsilon_{j,t}}\right|$. The resulting mean-adjusted price response measure, $Adj\left|\varepsilon_{j,t} / \sigma_{\varepsilon_{j,t}}\right|^{Mean} = \left|\varepsilon_{j,t} / \sigma_{\varepsilon_{j,t}}\right| - mean\left|\varepsilon_{j,t}^{Non-event} / \sigma_{\varepsilon_{j,t}}\right|$, captures the unusualness of the risk-adjusted absolute price movement on day t, relative to risk-adjusted absolute price movements occurring on non-event days. Positive values of $Adj\left|\varepsilon_{j,t} / \sigma_{\varepsilon_{j,t}}\right|^{Mean}$ indicate a larger price response to analyst forecasts than observed on non-event days – consistent with analysts' forecasts conveying material news to investors. Negative (or zero) values of $Adj\left|\varepsilon_{j,t} / \sigma_{\varepsilon_{j,t}}\right|^{Mean}$ indicate that price reactions on analyst forecast dates are smaller than (or equal to) reactions found on non-event days – suggesting that no material news is conveyed by analysts' forecasts.

We also report results where we adjust $\left| \varepsilon_{j,t} / \sigma_{\varepsilon_{j,t}} \right|$ by subtracting the *median* value of $\left| \varepsilon_{j,t} / \sigma_{\varepsilon_{j,t}} \right|$ calculated for a random sample of non-event days in year Y, $median \left| \varepsilon_{j,t}^{Non-event} / \sigma_{\varepsilon_{j,t}} \right|$. The resulting median-adjusted price response measure, $Adj \left| \varepsilon_{j,t} / \sigma_{\varepsilon_{j,t}} \right|^{Median} = \left| \varepsilon_{j,t} / \sigma_{\varepsilon_{j,t}} \right| - median \left| \varepsilon_{j,t}^{Non-event} / \sigma_{\varepsilon_{j,t}} \right|$, is interpreted similarly as the mean-adjusted price response measure. As previously discussed, median-adjusted price response measures will differ from mean-adjusted price response measures if the distribution of price responses on non-event days is skewed.

Our final set of results is based on a non-parametric comparison of price response measures for analysts' forecasts with the entire distribution of price response measures for the year in which the forecast occurred. Specifically, for each firm-year, we rank $\left| \varepsilon_{j,t} / \sigma_{\varepsilon_{j,t}} \right|$ from smallest to largest on a 1 to 100 scale. Thus, a trading day with the highest value of $\left| \varepsilon_{j,t} / \sigma_{\varepsilon_{j,t}} \right|$ receives a rank of 100, and a trading day with the lowest value of $\left| \varepsilon_{j,t} / \sigma_{\varepsilon_{j,t}} \right|$ has a rank of 1; we refer to these ranks as the percentile ranks, or "p-tile ranks" ($\left| \varepsilon_{j,t} / \sigma_{\varepsilon_{j,t}} \right|^{P-tile}$). The distribution of $\left| \varepsilon_{j,t} / \sigma_{\varepsilon_{j,t}} \right|^{P-tile}$ provides information about where the price response measure for an analyst forecast lies in the distribution of all price responses measures.

The non-event day population for firm j in year Y (used to calculate both $mean \left| \varepsilon_{j,t}^{Non-event} / \sigma_{\varepsilon_{j,t}} \right|$ and $median \left| \varepsilon_{j,t}^{Non-event} / \sigma_{\varepsilon_{j,t}} \right|$) consists of all trading days for firm j with no analyst forecasts, earnings announcements, ex dividend days, or management forecasts. In addition, we exclude trading days that are within one day of each of these confounding events. In selecting a random sample from the non-event day population, we choose the same number of non-event days as the number of forecast days in that firm-year (e.g., if we have 42 forecast dates for IBM in 1994, we select 42 non-event days from IBM's non-event population in 1994). Results are qualitatively similar if we use all non-event days, rather than a random sample, to compute the mean and median benchmarks.

3.2. Price response measured at the forecast date level

We begin by examining the market influence of analysts' forecasts at the forecast day level.

Specifically, we calculate $Adj\left|\varepsilon_{j,t}/\sigma_{\varepsilon_{j,t}}\right|^{Mean}$, $Adj\left|\varepsilon_{j,t}/\sigma_{\varepsilon_{j,t}}\right|^{Median}$ and $\left|\varepsilon_{j,t}/\sigma_{\varepsilon_{j,t}}\right|^{P-tile}$ for each forecast day t in firm-year $Y=1990-2003$. The sample consists, therefore, of all unique forecast dates for a given firm-year ($n=2,009,078$). Table 1, Panel A reports summary statistics of the price responses. We report mean-adjusted price responses for three samples: Column 1 shows results for all forecast days; Column 2 shows results for forecast days excluding days on which there are earnings announcements, ex-dividend days, or management earnings forecasts; and Column 3 shows results where we broaden the definition of confounding events to include other analysts' forecasts. Columns 4 and 5 show results for the median-adjusted price response measure and the p-tile rank measure, respectively.

We begin by noting that the sample decrements attributable to confounding events are not trivial. Inspection of the number of event days across Columns 1, 2 and 3 shows that we lose 15.7% of the initial sample (or 315,698 forecast days) when we exclude earnings announcements, ex dividend days and management forecast announcements, and we lose another 16.4% (328,584 forecast days) when we eliminate days when multiple analysts issued forecasts for the same firm. Thus, for even a relatively small set of information events, we find considerable (32%) contamination of analyst reports.

Turning to the results for the full sample of forecast days, Panel A, Column 1 shows that the mean value of $Adj\left|\varepsilon_{j,t}/\sigma_{\varepsilon_{j,t}}\right|^{Mean}$ indicates that the price response on forecast days is 0.1400 standard deviations higher than the average price response on non-event days (t-statistic = 235.14). However, the negative median value of $Adj\left|\varepsilon_{j,t}/\sigma_{\varepsilon_{j,t}}\right|^{Median}$, of -0.0723 (p-value of 0.0001), indicates that stock prices move *less* on forecast days than on the average of non-event days more than 50% of the time. The significant difference between the mean and median values of the price response measure suggests positive skewness, which is confirmed by the skewness statistic of 3.022, well above the expected value of zero for a normal distribution. The distribution of $Adj\left|\varepsilon_{j,t}/\sigma_{\varepsilon_{j,t}}\right|^{Mean}$ is also leptokurtic with

significantly positive kurtosis (higher peaks and longer tails than a normal distribution with the same standard deviation), as indicated by the kurtosis statistic of 18.390 (kurtosis for a normal distribution is 3). Panel A also shows that the fraction of forecast days with negative values of $Adj\left|\varepsilon_{j,t} / \sigma_{\varepsilon_{j,t}}\right|^{Mean}$ is 54.65%, significantly greater than chance at the 0.0001 level.

As discussed earlier, many forecast days coincide with other corporate events (such as earnings announcements) that are known to move stock prices. Therefore, measures of price response based on all forecast days without considering these confounding events are likely upwardly biased estimates of analysts' true price impact. Column 2 shows that mean and median values of $Adj\left|\varepsilon_{j,t} / \sigma_{\varepsilon_{j,t}}\right|^{Mean}$ are lower when these other announcements are eliminated: the mean value of $Adj\left|\varepsilon_{j,t} / \sigma_{\varepsilon_{j,t}}\right|^{Mean}$ declines from 0.1400 to 0.0990, and the median declines from -0.0723 to -0.0910. In unreported tests, we verify that these declines are significant at the 0.0001 level.

Multiple analysts issuing forecasts on the same day also affect inferences about individual forecast price response. Column 3 of Table 1 shows the incremental effect of excluding multiple analyst dates, above and beyond the effects of confounding events. Mean and median values of the price response measure are, again, smaller when multiple analyst forecasts days are excluded. For example, the mean price response declines to 0.0752 and the median price response declines to -0.1029. Relative to the mean and median values reported in Column 2 (which included multiple-analyst days), these values are significantly lower at the 0.0001 level (tests not reported).

We repeat our tests of $Adj\left|\varepsilon_{j,t} / \sigma_{\varepsilon_{j,t}}\right|^{Mean}$ for quarterly earnings announcement days (n=68,179) and management forecast days (n=13,142) for the sample firms over the sample period, 1990-2003. We choose these events for two reasons. First, the fraud on the market presumption is apparently applied without (much) question to earnings information (in the form of forecasts, warnings and actual

announcements) provided by issuers.¹⁷ Therefore, a finding that price responses to analysts' forecasts are distributed about the same as price responses to issuer-supplied earnings information would support the applicability of the fraud on the market presumption to the former. Second, Judge Rakoff's ruling that the fraud on the market presumption should *not* be applied to analysts' forecasts explicitly distinguished between statements of fact and statements of opinion (that is, between earnings and forecasts of earnings) and between information supplied by issuers and information supplied by third parties (that is, between management forecasts and analysts' forecasts). The implication of this distinction is that issuer-supplied information (whether opinions or facts) should have stronger and more pervasive stock market effects than information supplied by third parties. Our analyses shed light on whether this implication is evident in the distributions of price response measures.

Results of these tests, reported in Table 1, indicate reliably unusual price responses to earnings announcements (Panel B) and management forecasts (Panel C). For all earnings announcement days, the mean (median) value of $Adj|\varepsilon_{j,t} / \sigma_{\varepsilon_{j,t}}|^{Mean}$ is 0.5216 (0.1678); for all management forecast days, the mean (median) values are 1.2133 (0.5592). These mean and median price response measures are significantly larger (at the 0.0001 level, tests not reported) than the price response measures found for analysts' forecasts (reported in Panel A, of 0.1400 and -0.1029, respectively). In addition, for both earnings announcements and management forecasts, we find that a *minority* of event days have negative price response measures: 41.9% for earnings announcements and 30.5% for management forecasts. This result contrasts with the finding of negative price response measures for a *majority* of analyst forecast days (54.65%). We also find that the skewness and kurtosis statistics for the distributions of price responses to earnings announcements (2.381 and 9.042, respectively) and to management forecasts (1.875 and 4.064, respectively) are smaller, indicating less skewed and less kurtotic distributions, than documented for the distribution of price responses to analysts' forecasts (3.022 and 18.390, respectively). Finally, we note

¹⁷ However, in *Greenberg v. Crossroads Systems, Inc.* [No. 03-50311, slip. op., April 14, 2004] the Fifth Circuit Court of Appeals held that the fraud on the market presumption is not available for plaintiffs who cannot establish a causal relation between the allegedly defective issuer-initiated statements and the subsequent decline in the firm's share price when the alleged true information was revealed.

that we draw similar conclusions about the price responses to earnings announcements and management forecasts when we exclude event days with confounding events (Column 2 of Panels B and C).

Results based median-adjusted price response measures are reported in Column 4 of Table 1, for the least contaminated sample; recall that for these tests, the benchmark is the median (not the mean) standardized market model residual on non-event days. These results provide less evidence of skewness in price responses to analysts' forecasts; both the mean and median value of $Adj\left|\varepsilon_{j,t} / \sigma_{\varepsilon_{j,t}}\right|^{Median}$ are positive (53% of price responses to analysts' forecasts based on the median-adjusted measure are positive). The median-adjusted price responses to earnings announcements and management forecasts are also more positive, as evidenced by larger means, larger medians, and a smaller percentage of negative responses; for example, 66% of the median-adjusted price response measures for earnings announcements without confounding events, and 78% of the measures for management forecasts, are positive. These results indicate that median-adjusted positive price response measures are both more material and more pervasive for earnings announcements and management forecasts than for analysts' forecasts.

Results of p-tile rank tests are reported in Column 5 of Table 1 for the least contaminated samples. For analyst forecast days, the average value of $\left|\varepsilon_{j,t} / \sigma_{\varepsilon_{j,t}}\right|^{P-tile}$ is 51.09 and its median value is 51.56, indicating that the market impact of analysts' forecasts is just above the median of all price responses. Both the mean and median values are statistically significantly greater than 50 at the 0.0001 level; however, the *magnitudes* of the differences are very small. In comparison, the mean (median) value of $\left|\varepsilon_{j,t} / \sigma_{\varepsilon_{j,t}}\right|^{P-tile}$ for earnings announcements is 61.9 (67.5), well above 50; for management forecasts, the mean and median p-tile ranks are even higher at 73.0 and 86.9, respectively.

Overall, we believe the findings in Table 1 support the following inferences. First, the distribution of price responses to analysts' forecasts differs substantially from the distributions of price responses to earnings announcements and management forecasts. Earnings announcements and management forecasts have both larger and more pervasive effects. With respect to the size of the effect,

the average price response measure for analysts' forecasts is 75% smaller than the average price response measure for earnings announcements (0.1400 versus 0.5216) and 90% smaller than the average for management forecasts (0.1400 versus 1.2133). Second, and with respect to the pervasiveness of the effect, between 58% and 66% (depending on the price response measure examined) of earnings announcement days and between 70% and 78% of management forecast days are associated with larger price movements than are found on non-event days, as compared to between 43% and 53% of analyst forecast days. Finally, the magnitude of the price response to forecasts is inflated by contaminating events (i.e., the inclusion of forecast days that coincide with other significant corporate events) and clustering (i.e., the inclusion of forecast days on which multiple analysts issued forecasts about the same firm). When we control for contaminating events and report clustering, we find even larger differences between the price responses to analysts' forecasts and the responses to either earnings announcements or management forecasts, suggesting that confounding events have a greater influence on perceptions of the information content of analysts' reports than they do on perceptions of the information content of earnings announcements or management forecasts.

3.3. Market impact for other aggregations of forecast data

We aggregate the sample of forecast dates in various ways to provide evidence on specific questions concerning the market influence of analysts. We begin by aggregating forecast data at the *firm-year* level to answer the question of whether, as a group, analysts following a given firm have a significant impact on the market for that firm's stock. In answering this question, we restrict our sample to firm-years with a sufficient number of forecast days such that we can reliably assess whether the forecasts have a material price response. For this purpose, we select 10 forecast days as our cutoff; results based on minimum cutoffs of 15, 20 and 30 forecasts yield substantially similar inferences (not reported). For each firm-year with at least 10 unique forecast days, we calculate the mean and median values of the three price response measures, and we test whether these values are reliably different from zero at the 5% level, and separately, at the 10% level. Because our inferences are qualitatively similar regardless of the significance level, we discuss only the results for the 5% level.

Table 2, Panel A summarizes the results of these tests. Of the 38,837 firm-years with at least 10 forecast dates, Column 1 (which shows the results for the sample of all forecast days) shows that the mean value of $Adj\left|\varepsilon_{j,t} / \sigma_{\varepsilon_{j,t}}\right|^{Mean}$ is positive 77.0% of the time, but the median value is positive only 33.4% of the time. The fraction of firm-years where $Adj\left|\varepsilon_{j,t} / \sigma_{\varepsilon_{j,t}}\right|^{Mean}$ is *significantly* positive is, however, much smaller: 24.9% for mean values and 2.6% for median values. Columns 2 and 3 show the effects of confounding events and analyst report clustering. (These screens are placed on the original sample of forecast dates, reducing the number of forecast dates eligible for the screen requiring that each firm-year have at least 10 unique forecast days.) Relative to the full sample of 38,837 firm-years, the samples which exclude confounding events are modestly smaller with 35,826 firm-years (Column 2) and 35,379 firm-years (Column 3). As predicted, controlling for contaminating events results in weaker evidence of price impact. For example, when both confounding events and multiple forecast dates are excluded (Column 3), the results show that the average (median) value of $Adj\left|\varepsilon_{j,t} / \sigma_{\varepsilon_{j,t}}\right|^{Mean}$ is significantly positive about 12.4% (1.3%) of the time. This means that one *cannot* reject the null hypothesis of no price impact at the 5% level for 87.6% (98.7%) of the firm-years.

Column 4 of Panel A reports the results of firm-year based on $Adj\left|\varepsilon_{j,t} / \sigma_{\varepsilon_{j,t}}\right|^{Median}$. Similar to Panel A of Table 1, results based on the median-adjusted price response measure generally show more evidence of significant price movements in response to analysts' forecasts. In particular, the mean (median) value of $Adj\left|\varepsilon_{j,t} / \sigma_{\varepsilon_{j,t}}\right|^{Median}$ is positive 89.1% (60.6%) of the time, and is significantly greater than zero 44% (10.8%) of the time. Column 5 reports the value of $\left|\varepsilon_{j,t} / \sigma_{\varepsilon_{j,t}}\right|^{P-tile} - 50$, where 50 is, by construction, the median percentile rank. For forecast days aggregated at the firm-year level, about 60.2% (57.5%) of observations have a mean (median) value of $\left|\varepsilon_{j,t} / \sigma_{\varepsilon_{j,t}}\right|^{P-tile} - 50$ that is positive; of these, only 3.5% (1.9%) are reliably positive at the 5% level.

Our next aggregation of the data is intended to address the significance of individual analysts' influence on the share price of a given firm they choose to follow. Our goal here is to document the percentage of analysts following a given firm who issue reports that are associated with reliably positive price response measures. Because an analyst's price impact may differ across the stocks he follows, we evaluate the market influence of *analyst-firm* pairings. Our analysis here is similar to that reported in Panel A, except that instead of using the firm-year as the unit of analysis, we use the analyst-firm as the unit of analysis. For this purpose, we group all forecast dates for a given analyst-firm across the sample period, Y=1990-2003. As in our investigation of firm-years, we restrict the sample to those analyst-firm pairs with at least 10 unique forecast dates. In total, our sample has 101,780 analyst-firm pairs, before any further restrictions for confounding events (which reduce the sample to 80,103 analyst-firm pairs) and multiple forecast dates (which reduce the sample further to 53,180 analyst-firm pairs).

Panel B shows the percentage of the analyst-year observations where the mean and median values of the price response measures are positive and, separately, reliably positive. Our results are similar to those documented in Panel A (for firm-year aggregations). Specifically, while we find that the majority of mean values of $Adj\left|\varepsilon_{j,t} / \sigma_{\varepsilon_{j,t}}\right|^{Mean}$ are positive (between 65.1% and 84.4%, with the range depending on the controls for confounding events and clustering), median values of $Adj\left|\varepsilon_{j,t} / \sigma_{\varepsilon_{j,t}}\right|^{Mean}$ are positive less than half the time (between 29.3% and 48%), and none of the measures is reliably positive (at the 5% level) more than 25.6% of the time. For the sample that controls best for other events (Column 3), the percentage of reliably positive values of $Adj\left|\varepsilon_{j,t} / \sigma_{\varepsilon_{j,t}}\right|^{Mean}$ is 3.9% (based on mean) and 0.5% (based on median). In other words, if the standard to be an influential analyst for a given firm requires that the average value of $Adj\left|\varepsilon_{j,t} / \sigma_{\varepsilon_{j,t}}\right|^{Mean}$ on that analyst's forecast days is significantly (at the 5% level) higher than on non-event days, over 96% of analysts would *not* be influential. Results based on

$Adj\left|\varepsilon_{j,t} / \sigma_{\varepsilon_{j,t}}\right|^{Median}$ (Column 4) and $\left|\varepsilon_{j,t} / \sigma_{\varepsilon_{j,t}}\right|^{P-tile} - 50$ (Column 5) show similar inferences. In particular,

while the majority of mean and median price responses to analysts' forecasts are positive, the incidence of reliably (at the 5% level) positive price responses is small: between 3.9% and 22.9% for

$$Adj\left|\varepsilon_{j,t} / \sigma_{\varepsilon_{j,t}}\right|^{Median} \text{ and between 2.2\% and 7.7\% for } \left|\varepsilon_{j,t} / \sigma_{\varepsilon_{j,t}}\right|^{P-tile} - 50.$$

Our final analyses aggregate forecasts at the *analyst-year* level and the *analyst-firm-year* level.

The former (Panel C) speak to how a given analyst's forecasts in year Y for all firms s/he covers influence the market; the latter (Panel D) speak to how a given analyst's forecasts about firm X in year Y influence the market. Again, we require a minimum of 10 unique forecast dates in year Y, for either aggregation, to be included in these tests. As a result, our samples – particularly for the analyst-firm-year level tests – contain only analysts who are active forecasters. Results for these aggregations are similar to those reported in Panels A and B. For example, for the sample that controls best for other events (Column 3 results), we find that between 0.5% and 10.4% of analyst-year price responses based on the

$Adj\left|\varepsilon_{j,t} / \sigma_{\varepsilon_{j,t}}\right|^{Mean}$ metric are reliably positive (at the 5% level); for the analyst-firm-year tests, similar figures range between 0.7% and 2.4%. We draw similar inferences using the other two price metrics,

$$Adj\left|\varepsilon_{j,t} / \sigma_{\varepsilon_{j,t}}\right|^{Median} \text{ and } \left|\varepsilon_{j,t} / \sigma_{\varepsilon_{j,t}}\right|^{P-tile} - 50.$$

Consistent with our previous findings, these results indicate that the majority of price responses to analysts' forecasts are not reliably unusual, relative to price movements on non-event days. This conclusion is most pronounced for results based on the mean-adjusted price response metric and the percentile rank price response metric. We further note that even if the threshold for statistical significance is weakened from 5% to 10%, the percentage of reliably unusual price responses to analysts' forecasts remains low: less than 17% based on mean values and about 1% based on median values (Panel C, Column 3, the least contaminated sample).

3.4. Summary and extensions

Our findings indicate that reliably unusual price movements in response to analysts' forecasts are the exception not the norm, particularly when unusualness is measured relative to the *average*

standardized price movement on non-event days. This pattern is evident both for individual forecast day responses and for all methods of aggregating forecasts that we consider. The low frequency of reliably unusual price response measures is most pronounced for samples that exclude days with confounding news events and other analysts' forecasts. Overall, we interpret these results as indicating that most analysts, and most analysts' forecasts, have no material influence on share values, as reflected by values of our price response metrics that are not reliably greater than zero.

We probe this result along two dimensions that have particular relevance to securities litigation over allegedly defective statements. The first is that the fraud on the market claim applies only to securities that can be shown to be traded in an efficient market. The concern here, as it relates to our tests, is whether our finding of relatively small analyst influence is driven by firms that might fail to meet the market efficiency test. We investigate this possibility by partitioning our sample along several dimensions intended to capture differences in the efficiency of the market for a given stock: market capitalization, analyst following and exchange listing. Larger firms, firms followed by more analysts, and firms listed on the NYSE/AMEX are generally viewed as having more efficient markets than smaller firms, firms followed by few or no analysts, and firms traded on the NASDAQ. For market capitalization and analyst following, we rank firms each year and form deciles; we then repeat our tests on each decile. For exchange listing, we repeat our tests on the sample of firms listed on NYSE/AMEX versus NASDAQ. Results of these tests (not reported) show no evidence that our findings are concentrated in, or driven by, the smallest firms, the least-followed firms, or firms traded on the NASDAQ.

The second dimension we consider is the external reputation of some analysts, as reflected by their identification as *Institutional Investor* All American analysts. Based on prior research showing larger price reactions to All-American analysts' forecasts than non-All-American analysts' forecasts (Stickel [1992]; Gleason and Lee [2003]), we would expect All-American analysts to have greater market impact. We examine this expectation by investigating the overlap between analysts identified by our price response measure as being influential in year Y and analysts named to the *Institutional Investor* All

American research team in year Y, Y=1990-2003. We define an analyst as being influential in year Y if the price response measure calculated at the analyst-year level is reliably positive at the 5% level.

Table 3 shows that of the total sample of 27,230 analyst-year observations (the same sample as examined in Panel C, Table 2), 12,005 observations are influential based on $Adj|\varepsilon_{j,t}/\sigma_{\varepsilon_{j,t}}|^{Mean}$. Of these 12,005 observations, 10,832 have Zacks data on the analyst's name, and 1,173 do not (these observations are from First Call). The total number of unique All-American analyst-years in our sample, with Zacks data and the required 10 or more forecasts, is 1,683. The overlap between the influential analyst-year sample and the All-American sample is 1,031 analyst-years. We note first that these results indicate that most analysts who are influential based on our price response measure (n=12,005) are not All-American Analysts (n=1,698). Our second observation is that conditional on being an All American analyst in year Y, there is a 61.3% (1,031/1,683) probability that this analyst-year is influential based on

$Adj|\varepsilon_{j,t}/\sigma_{\varepsilon_{j,t}}|^{Mean}$. This percentage is larger than the unconditional probability of being influential of 44.1% (12,005/27,230), indicating that the fact that an analyst is an *Institutional Investor* All-American in year Y has an incremental effect on the likelihood that he is influential. The magnitude of this incremental probability declines as the samples become less contaminated; for the sample that excludes confounding events, the incremental probability is about 18% (44% versus 26%, Column 2), and for the least contaminated sample, the incremental probability is 1.23% (11.70% versus 10.47%, Column 3).

4. Additional Sensitivity Tests

We examine the sensitivity of our results to several research design choices. Our first set of tests examines alternative price response measures. To probe the effects of standardizing by returns volatility, we repeat our tests using an unstandardized price response measure, $Adj|\varepsilon_{j,t}| = |\varepsilon_{j,t}| - mean|\varepsilon_{j,t}^{Non-event}|$. While the pattern of results (not tabled) is similar to reported results, the skewness parameter for the unstandardized measure is about 9 and the kurtosis score exceeds 300, substantially higher than the

parameters for the standardized measure of about 3 (skewness) and 18 (kurtosis). We also examine the effect of using size-adjusted returns as opposed to market model residuals (the size-adjusted benchmark is also used by Frankel, Kothari and Weber [2004]). The specific size-adjusted measure we use is:

$SizeAdj|Ret_{j,t}| = |R_{j,t} - R_{size,t}| - mean|(R_{j,t} - R_{size,t})^{Non-event}|$, where $R_{j,t}$ is the return on forecast day t,

$R_{size,t}$ is the return of firm j's size portfolio, $(R_{j,t} - R_{size,t})^{Non-event}$ is firm j's size-adjusted return on non-event day t. Non-event days are defined as in our previous tests. The results of these tests (not tabled) are nearly identical to those reported in Tables 1 and 2. Based on the combined evidence, we conclude that our findings are robust to specification choices for the price response measure.

Our second analysis examines the effects of basing the price response measure on a comparison of standardized residuals on forecast days versus non-event days. As discussed in note 1, the phrase “non-event” does not mean the complete absence of news; indeed, the existence of price reactions on non-event days suggests that *something* happened (otherwise prices would not have moved).¹⁸ By non-event, we mean the absence of disclosures of significant news about earnings and dividends, namely, earnings announcements, management forecasts and ex-dividend days. However, disclosures of other types of news (e.g., announcements of new products, joint ventures, management changes, mergers) would be expected to occur on non-event days. Our measure of the forecast day price response assumes, therefore, that the incidence of such disclosures is the same on non-event days as it is on forecast days. If this assumption is correct, then the non-event day price response benchmark controls for other information events, allowing us to uniquely capture the price effects of analysts' forecasts. If there is more news on forecast days than on non-event days (as prior research suggests), our tests are biased toward showing larger and more pervasive positive price responses on forecast days. Finally, if there is less news on forecast days than non-event days, then our tests are biased toward the results we document – smaller and less pervasive evidence of positive price responses on forecast days.

¹⁸ A public disclosure is not a necessary condition for prices to move, since price movements could result from trading by traders with private information.

In determining the appropriateness of this assumption, we begin by noting that there is no other way to assess the unusualness (that is, the significance) of the share price response measure except through a comparison with the empirical distribution.¹⁹ Therefore, our findings shed light only on whether share price movements on analyst forecast days are larger or smaller in magnitude than share price movements on other days. If the movement in share prices on non-event days is a good benchmark for normal price movements (that is, price movements that occur in the absence of an investor response to an analyst report), then the issue is an empirical one: how frequent are other disclosures not explicitly controlled for by our tests.

We investigate this issue by randomly selecting 500 non-event day-firm pairings and 500 forecast day-firm pairings and searching the Factiva database for disclosures made by or about these firms on these days.²⁰ For the non-event day sample, we find disclosures for 81 days (16% of the sample); for the forecast day sample, the number of disclosure days is 136 (27%). These results indicate that disclosures are nearly 70% more likely (27% versus 16%) on forecast days than on non-event days. Our search also revealed that, for both samples, disclosures are disproportionately associated with very large firms. In particular, although S&P 500 firms comprise 28% of the non-event day random sample, they represent 53% (43 of 81) of the non-event days with disclosures; for the forecast day random sample, S&P 500 firms represent 24% of forecast days and 35% (47 of 136) of disclosure days. Given the breadth of our sample, it is unlikely that the forecasts associated with S&P 500 firms drive our results concerning the low incidence of reliably unusual price responses to forecasts. In unreported tests, we verify this is the

¹⁹ Specifically, because $\left| \varepsilon_{j,t} / \sigma_{\varepsilon_{j,t}} \right|$ is not normally distributed, it is not appropriate to compare it to, for example, standard cutoff values such as 1.96 (the value of a t-statistic with a confidence level of 95%). Notwithstanding this concern, we calculated the percentage of forecast days where $\left| \varepsilon_{j,t} / \sigma_{\varepsilon_{j,t}} \right| > 1.96$. For the least contaminated sample (full sample), this percentage is about 6% (7.5%); in comparison, $\left| \varepsilon_{j,t} / \sigma_{\varepsilon_{j,t}} \right| > 1.96$ for about 4% of non-event days.

²⁰ The Factiva database has broad coverage: more than 1,500+ global and local newspapers, 3,200+ magazines (including industry-specific journals and newsletters), 500+ newswire services (including Dow Jones, Reuters and The Associated Press), 160+ Media Programs (with transcripts from major radios and televisions such as BBC, ABC, CBS and NBC), and nearly 4,000 web sites in more than 20 languages.

case by repeating our tests after excluding all forecasts pertaining to S&P 500 firms (results are similar and are not reported).

In summary, while we can not rule out the possibility that our benchmark for establishing the unusualness of price responses to analysts' forecasts is affected by price movements associated with disclosures on non-event days that are not controlled for by our tests, we believe this possibility is unlikely to explain our results, for two reasons. First, by eliminating from the non-event day population days with earnings announcements, management forecasts and ex dividends, our tests rule out reactions to disclosures which are known to have large share price effects. Second, our analysis of disclosures on random samples of non-event days and forecast days indicates that non-event days are less likely to contain other disclosures than are forecast days; this finding is consistent with prior research which shows a tendency for analysts' reports to cluster around news events. The latter result implies that, if anything, our tests are biased toward finding larger price responses on forecast days than on non-event days; the fact that our results tend to show the opposite suggests that the benchmark does not drive the results.

Our third sensitivity test examines whether our results are driven by the sign of the news (good versus bad) conveyed by the forecast. We sign the direction of the news released on the forecast day using the sign of the abnormal return on that day (i.e., the sign of $\varepsilon_{j,t}$). The advantages of this approach are twofold: 1) it does not require us to choose which piece of information in the analyst's report conveyed the most information (this is an issue if some components of the report convey good news while others convey bad news); and 2) it does not require assumptions about investors' expectations (i.e., should a revised earnings forecast be compared to the analyst's prior earnings forecast for this firm or to the consensus forecast for this firm?). We repeat our tests in Panel A, Table 1 after partitioning the sample based on whether the forecast day market model residual ($\varepsilon_{j,t}$) is positive or negative. Results (not reported) are similar to those for the combined sample of forecasts. Specifically, for both good news forecasts and bad news forecasts, we find that mean value of $Adj|\varepsilon_{j,t} / \sigma_{\varepsilon_{j,t}}|^{Mean}$ is positive (0.1170 for good news, 0.0361 for bad news, for the least contaminated sample), the median is negative (-0.0856 for

good news, -0.1172 for bad news), and the majority of forecast days have smaller price responses than on non-event days (55.5% for good news, 58.2% for bad news). Based on this evidence, we conclude that inferences drawn for the full sample are not driven either by good news or by bad news.

As a fourth test, we investigate whether our results are driven by the type of forecast issued: k = earnings forecast, long term growth forecast or stock recommendation. We define firm j 's forecast day t as reflecting a forecast of type k if at least one forecast of that type is made on day t . Using this definition, the k samples of forecast days are not independent because we include forecast days in multiple samples if two or more types of forecasts are issued on the same day. Repeating our Table 1 tests on each of the k samples (not tabulated) show patterns similar to the full sample: price response measures have a positive mean, a negative median, and 55% or more price responses are negative.

Our fifth sensitivity test examines the market response to analysts' forecasts as captured by reliably unusual trading volume responses. Our volume metric is calculated similar to the price response measure; specifically, the adjusted abnormal trading volume measure, $Adj_Vol_{j,t}$ is the difference between the standardized trading volume (in shares) on the forecast day and the average standardized trading volume on randomly selected non-event days (where the non-event day sample is the same as for the price impact measure). Like the standard deviation of the absolute residuals, the standard deviation of trading volume is calculated over all trading days for firm j in year Y . Results (not reported) based on $Adj_Vol_{j,t}$ yield qualitatively similar inferences as those based on $Adj\left|\varepsilon_{j,t} / \sigma_{\varepsilon_{j,t}}\right|^{Mean}$.

Our final sensitivity check investigates the effect of reiterations. The samples used in our previous tests include *all* earnings forecasts, growth rate forecasts and stock recommendations issued by analysts over $Y=1990-2003$, regardless of whether analysts revised these forecasts or reiterated previous information. Frankel et al. also include both revisions and reiterations in their sample. The argument for including reiterations is that reiterations have the potential to convey information to the market.²¹ In the

²¹ Consistent with the view that reiterations convey information, we note that many of the alleged misrepresentations in the investment banking scandals of 2003 involved claims that analysts failed to revise their forecasts when they possessed information (and held opinions) which ran counter to their stated beliefs. The claim here was that

case of stock recommendations, it is straightforward to see why a reiteration might convey information: it represents the analyst's belief that the stock continues to be mispriced. For example, a reiterated Buy recommendation indicates the analyst's belief that the stock price continues to be below the analyst's estimate of intrinsic value; we might expect investors to respond by bidding up the price of the stock.²² Reiterated earnings forecasts may convey news in two ways. On the one hand, if the news content of an earnings forecast is determined based on its deviation from the consensus rather than its deviation from the analyst's prior earnings forecast, a reiterated forecast will convey news if it differs from the consensus. On the other hand, if the news content of the earnings forecast is determined by reference to the analyst's prior forecast, a reiteration may still convey news because of its effect on the consensus. To see why, note that most measures of the consensus reported by the popular press and included in data bases use the mean (or median) value of all forecasts (for a given firm-quarter) issued over a recent interval, such as the past 30-days. Consequently, a reiterated forecast alters the set of forecasts underlying the consensus, and therefore shifts the consensus forecast – which is itself a news event.

To gauge the sensitivity of our findings to reiterations, we repeat our tests on the subset of forecast observations containing revisions, i.e., we exclude reiterations. In determining whether a forecast is a reiteration, we assume that forecasts with no prior forecast information available are new forecasts, not reiterations; results are qualitatively similar if we eliminate these observations. Across all methods of aggregating forecasts, the results (not reported) indicate the same patterns for the samples that exclude reiterated forecasts as found for the samples that include them. We conclude that the inclusion of reiterated forecasts in our main tests does not explain the general absence of reliably unusual price responses to analysts' forecasts.

reiterated forecasts maintained or supported share prices which would have been lower if the true opinions of analysts had been expressed in their reports.

²² Francis and Soffer [1997] find significant positive reactions to analysts' reiterations of buy recommendations, but do not find significant reactions to their reiterated sell recommendations.

5. *Summary and Conclusions*

We provide evidence on the pervasiveness of reliably unusual price responses to analysts' forecasts (specifically, earnings forecasts, growth forecasts and stock recommendations). Our study is motivated by differing views about whether the fraud on market presumption of reliance, which embodies an expectation of material price impact, should be extended to analysts' forecasts. Our evidence is based on the properties of the distributions of price responses to analysts forecasts and on comparisons of those distributions to the distributions of price responses to earnings announcements and management forecasts.

With regard to the properties of the distributions of price responses to analysts forecast, our results support prior evidence of a reliably unusual *on average* price response to analysts' forecasts, but this result is not representative of the majority of price responses which are *smaller* than the price response observed on average non-event days; that is, the distribution is skewed. Distributions of price response measures for aggregations of forecasts at the firm-year level, analyst-firm level, analyst-year level, and analyst-firm-year level are also skewed and if anything, the price response measures for these aggregations show even less evidence that analysts' forecasts are associated with share price movements that are reliably unusual compared to average non-forecast days. With regard to comparisons between price responses to analysts' forecasts responses and price responses to either earnings announcements and management forecasts, we find that the former are systematically less material and less pervasive than the latter. The fraud on the market presumption has often been applied to both earnings announcements and management forecasts; if the criterion for extending the presumption to analysts forecasts is similarity in the materiality and pervasiveness of price responses, our results would not support this extension.

We interpret the weight of the evidence as indicating that for the most part, analysts' representations do not matter for share pricing, insofar as we find that most are not associated with price responses that are reliably unusual compared to price movements observed on average non-event days. To the extent *prima facie* evidence of a reasonably quantifiable nature is required to support the fraud on the market presumption that a particular analyst's statements about a given firm materially affected that firm's share price, we believe our results support the inference that the presumption will likely be applied

sparingly in the context of allegedly fraudulent or misleading analyst disseminations. That is, our results are consistent with the view that plaintiffs would have to show, in their specific analyst litigation context, that the analyst's statements and reports that are the subject of the litigation were associated with a measurable share price response; they should not be entitled to a presumption that this is the case. Our results do not, of course, speak to the likelihood that such a demonstration would or would not be successful because the demonstration of share price impact would be highly context-specific and fact-intensive.

Our tests have focused on whether analysts' forecasts systematically convey news, measured by whether price movements on forecast days are reliably different from price movements on average non-event days. Our results do not imply that analysts' forecasts do not have value, for two reasons. First, our measure of price impact is whether the price response on forecast days is statistically distinguishable from the average price response observed on non-event days. Thus, a careful interpretation of our findings would be that the majority of analysts' forecasts do not convey *any more* (and often convey less) information to the market than is conveyed, on average, on days where these other events did not occur. Second, analysts may have value by improving the efficiency of the market for a stock. Piotroski and Roulstone [2004] show that greater analyst forecasting activity (measured as the number of forecast revisions made during the year) is positively associated with the synchronicity of the firm's stock returns (measured as the amount of variation in the firm's returns explained by current and lagged market returns and industry returns). The authors interpret this finding as indicating that, on average, analyst forecasting activity increases the amount of industry-level information that is impounded in stock prices. Our tests do not examine the extent to which individual analysts enhance the flow of information into prices, and therefore, do not speak to the value that may be added by analysts via improved efficiency.

Table 1
Price Impact of Analysts' Forecasts, Earnings Announcements and Management Forecasts^a

	Col. 1	Col. 2	Col. 3	Col. 4	Col. 5
	Excluding event days				
	All event days	with confounding events	Excluding event days with more than 1 analyst report		
Panel A: Analysts' forecasts					
Price response measure	$Adj \epsilon/\sigma ^{Mean}$	$Adj \epsilon/\sigma ^{Mean}$	$Adj \epsilon/\sigma ^{Mean}$	$Adj \epsilon/\sigma ^{Median}$	$ \epsilon/\sigma ^{P-tile}$
Individual event days	2,009,078	1,693,380	1,364,796	1,364,796	1,364,796
Mean	0.1400	0.0990	0.0752	0.2215	51.0972
Standard deviation	0.8440	0.7757	0.7317	0.7328	29.0750
t-statistic	235.14	166.03	120.01	353.03	44.09
Median	-0.0723	-0.0910	-0.1029	0.0425	51.5625
p-value	0.0001	0.0001	0.0001	0.0001	0.0001
% negative	0.5465	0.5603	0.5690	0.4686	0.4873
Skewness	3.022	2.92	2.59	2.57	-0.03
Kurtosis	18.390	18.91	15.82	15.61	-1.21
Panel B: Earnings announcements	All event days	Excluding event days with confounding events			
Price response measure	$Adj \epsilon/s ^{Mean}$	$Adj \epsilon/\sigma ^{Mean}$	$Adj \epsilon/\sigma ^{Median}$	$ \epsilon/\sigma ^{P-tile}$	
Individual event days	68,179	62,849	62,849	62,849	
Mean	0.5216	0.4950	0.6423	61.9390	
Standard deviation	1.2321	1.2002	1.1984	30.5050	
t-statistic	110.54	103.40	134.36	98.12	
Median	0.1678	0.1522	0.3010	67.4600	
p-value	0.0001	0.0001	0.0001	0.0001	
% negative	0.419	0.425	0.343	0.353	
Skewness	2.381	2.377	2.365	-0.421	
Kurtosis	9.042	9.180	9.104	-1.121	
Panel C: Management forecasts	All event days	Excluding event days with confounding events			
Price response measure	$Adj \epsilon/\sigma ^{Mean}$	$Adj \epsilon/\sigma ^{Mean}$	$Adj \epsilon/\sigma ^{Median}$	$ \epsilon/\sigma ^{P-tile}$	
Individual event days	13,142	7,977	7,977	7,977	
Mean	1.2133	1.4486	1.5926	73.0480	
Standard deviation	1.9453	2.1370	2.1348	30.2325	
t-statistic	71.5015	60.5300	66.6296	68.0893	
Median	0.5592	0.7121	0.8571	86.9047	
p-value	0.0001	0.0001	0.0001	0.0001	
% negative	0.305	0.282	0.228	0.239	
Skewness	1.875	1.662	1.659	-0.093	
Kurtosis	4.064	2.889	2.871	-0.470	

Sample description and variable definitions: The sample used in Panel A consists of trading days between 1990-2003 when analysts' issue earnings forecasts, growth forecasts and stock recommendations as reported by either Zacks or First Call. The sample of earnings announcements (Panel B) consists of all quarterly earnings announcement dates, for the firms with forecast data. The management forecast sample (Panel C) consists of all management forecast dates (obtained from First Call), for the firms with forecast data. We treat the event day (forecast day, earnings announcement day, or management forecast day) as the unit of observation. In column 2, we restrict the sample by excluding event days where other news events occurred on the same day. In columns 3-5, we show results after excluding forecast days when multiple analysts have issued reports.

Variable definitions: $Adj\left|\varepsilon_{j,t} / \sigma_{\varepsilon_{j,t}}\right|^{Mean}$ = the absolute value of the standardized market model residual on the forecast day less the mean absolute value of the standardized market model residual for non-event days. $Adj\left|\varepsilon_{j,t} / \sigma_{\varepsilon_{j,t}}\right|^{Median}$ = the absolute value of the standardized market model residual on the forecast day less the median absolute value of the standardized market model residual for non-event days. $\left|\varepsilon_{j,t} / \sigma_{\varepsilon_{j,t}}\right|^{P-tile}$ = the rank of the standardized share price response on event day t relative to all trading day price responses in year t, scaled from 1 to 100; values above (below) 50 indicate greater than (less than) median response.

^a Panel A reports information on the distributional properties of the three price response metrics for analysts' forecasts. Panels B and C report similar information for earnings announcements and management forecasts, respectively. The benchmark for comparing the mean and median adjusted price response measures ($Adj\left|\varepsilon_{j,t} / \sigma_{\varepsilon_{j,t}}\right|^{Mean}$ and $Adj\left|\varepsilon_{j,t} / \sigma_{\varepsilon_{j,t}}\right|^{Median}$) is zero; the benchmark for the p-tile rank variable ($\left|\varepsilon_{j,t} / \sigma_{\varepsilon_{j,t}}\right|^{P-tile}$) is 50.

Table 2
Price Impact of Analysts' Forecasts, Aggregated at Various Levels^a

	Col. 1	Col. 2	Col. 3	Col. 4	Col. 5
	Excluding event days				
	All event days	with confounding events	Excluding event days with more than 1 analyst report		
Price response measure	$Adj \epsilon/\sigma ^{Mean}$	$Adj \epsilon/\sigma ^{Mean}$	$Adj \epsilon/\sigma ^{Mean}$	$Adj \epsilon/\sigma ^{Median}$	$ \epsilon/\sigma ^{P-tile -50}$
Panel A: Firm-year level (n)	38,837	35,826	35,379	35,379	35,379
% where mean $Adj \epsilon/\sigma > 0$	0.770	0.712	0.669	0.891	0.602
and significant at 5% level	0.249	0.170	0.124	0.440	0.035
and significant at 10% level	0.340	0.251	0.195	0.545	0.070
% where median $Adj \epsilon/\sigma > 0$	0.334	0.286	0.271	0.606	0.575
and significant at 5% level	0.026	0.017	0.013	0.108	0.019
and significant at 10% level	0.044	0.030	0.023	0.157	0.042
Panel B: Analyst-firm level (n)	101,780	80,103	53,180	53,180	53,180
% where mean $Adj \epsilon/\sigma > 0$	0.844	0.768	0.651	0.896	0.570
and significant at 5% level	0.256	0.110	0.039	0.229	0.043
and significant at 10% level	0.362	0.193	0.086	0.357	0.077
% where median $Adj \epsilon/\sigma > 0$	0.480	0.363	0.293	0.612	0.554
and significant at 5% level	0.043	0.012	0.005	0.039	0.022
and significant at 10% level	0.069	0.024	0.011	0.072	0.044
Panel C: Analyst-year level (n)	27,230	26,092	23,409	23,409	23,409
% where mean $Adj \epsilon/\sigma > 0$	0.876	0.819	0.701	0.942	0.589
and significant at 5% level	0.441	0.260	0.104	0.463	0.067
and significant at 10% level	0.525	0.355	0.166	0.582	0.108
% where median $Adj \epsilon/\sigma > 0$	0.400	0.285	0.222	0.649	0.567
and significant at 5% level	0.062	0.013	0.005	0.076	0.041
and significant at 10% level	0.087	0.026	0.011	0.119	0.071
Panel D: Analyst-firm-year level (n)	103,421	54,726	11,965	11,965	11,965
% where mean $Adj \epsilon/\sigma > 0$	0.760	0.670	0.585	0.815	0.543
and significant at 5% level	0.089	0.049	0.024	0.129	0.031
and significant at 10% level	0.177	0.105	0.062	0.234	0.058
% where median $Adj \epsilon/\sigma > 0$	0.476	0.387	0.322	0.567	0.533
and significant at 5% level	0.022	0.012	0.007	0.035	0.012
and significant at 10% level	0.041	0.023	0.013	0.059	0.025

Sample description: The sample in Panel A consists of all *firm-years* with at least 10 analyst forecasts. The sample in Panel B consists of all *analyst-firm* observations with at least 10 analyst forecasts. The sample in Panel C consists of all *analyst-years* with at least 10 analyst forecasts. The sample in Panel D consists of all *analyst-firm-years* with at least 10 analyst forecasts. Column 2 further restricts each sample by excluding forecast days where other news events occurred (earnings announcement, management forecasts or ex-dividend announcements) on the same day or within one day before or after. The far right column shows results where we further exclude forecast days when multiple analysts have issued reports.

Variable definitions: $Adj\left|\varepsilon_{j,t} / \sigma_{\varepsilon_{j,t}}\right|^{Mean}$ = the absolute value of the standardized market model residual on the forecast day less the mean absolute value of the standardized market model residual for non-event days. $Adj\left|\varepsilon_{j,t} / \sigma_{\varepsilon_{j,t}}\right|^{Median}$ = the absolute value of the standardized market model residual on the forecast day less the median absolute value of the standardized market model residual for non-event days. $\left|\varepsilon_{j,t} / \sigma_{\varepsilon_{j,t}}\right|^{P-tile} - 50$ = the rank of the standardized share price response on event day t relative to all trading day responses in year t (scaled from 1 to 100, 100 being the largest price response) less 50; values above (below) zero indicate greater than (less than) median response.

^a In each panel, we report the percentage of observations (where an observation is defined by the way the sample is aggregated) where the mean, and separately median, of the noted price response measure is positive, reliably positive at the 5% level, and reliable positive at the 10% level. For example, Panel A reports the number of times where the mean (median) *firm-year* response to analysts' forecasts is reliably positive. Panel B reports similar information for forecasts aggregated at the *analyst-firm* level; Panel C for the *analyst-year* level; and Panel D for the *analyst-firm-year* level.

Table 3
The Overlap Between Influential Analysts and *All-American* Analysts^a

	Col. 1	Col. 2	Col. 3
	All event days Adj ε/σ ^{Mean}	Excluding event days with confounding events Adj ε/σ ^{Mean}	Excluding event days with > 1 analyst report Adj ε/σ ^{Mean}
Price response measure			
Analyst-years with ≥ 10 forecast days	27,230	26,092	23,409
# Influential analyst-years from First Call	1,173	950	588
# Influential analyst-years from Zacks	10,832	5,857	1,864
Total # Influential analyst-years	12,005	6,807	2,452
# All American analyst-years in Zacks	1,698	1,698	1,698
# All-American analyst-years in Zacks with ≥ 10 forecasts	1,683	1,673	1,598
# All-American analyst-years (in Zacks) that are influential	1,031	736	187
Conditional probability of being influential	61.26%	43.99%	11.70%
Unconditional probability of being influential	44.09%	26.09%	10.47%

Sample description: The sample consists of all *analyst-years* with at least 10 analyst forecasts. Column 2 further restricts the sample by excluding forecast days where other news events occurred (earnings announcement, management forecasts or ex-dividend announcements) on the same day or within one day before or after. The far right column shows results where we further exclude forecast days when multiple analysts have issued reports.

Variable definitions: $Adj|\varepsilon_{j,t}/\sigma_{\varepsilon_{j,t}}|^{Mean}$ = the absolute value of the standardized market model residual on the forecast day less the mean absolute value of the standardized market model residual for non-event days.

^a We examine the overlap between All-American status and “Influential” analysts. Whether an analyst is an All-American in year Y (or not) is determined by reading and coding the *Institutional Investor* All-American analyst lists for 1990-2003. An analyst-year is identified as Influential if the mean value of $Adj|\varepsilon_{j,t}/\sigma_{\varepsilon_{j,t}}|^{Mean}$ for that analyst-year is significant at the 5% level. We separate Influential analysts by the source of the forecast data (Zacks or First Call) because only Zacks contains the identities of the analysts which are needed for examining overlap with All-American status.

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